stand under what conditions it can become an excavating one, and how it can hollow out basins, &c.

When ice moves away from the slope which gives impetus to a glacier, the motion rapidly slackens and presently stops. The distance travelled over the level ground is a function of the weight of the glacier, of the amount of the slope, the friction of its bed, &c.; *i.e.* of the elements making up the *vis a tergo*; but in the very largest glaciers, so far as observation goes, the motion rapidly ceases on level ground. This is the evidence wherever the phenomenon has been observed and reported upon.

This being so, I altogether question not only the arguments of those who champion the excavation of lake basins by ice, but also of that larger school who invoke movements of ice over level plains of many hundreds of miles in extent in order to explain the drift phenomena. They do it, so far as I know, on the ground that they cannot appeal to any other cause without doing injustice to that modern metaphysical bogey, "The Doctrine of Uniformity." My small boy might just as well, on the same principle, attribute the excavation of his porringer to the porridge in the bowl. True rock basins were no doubt very largely due to the weathering of rocks which exfoliate, and whose structure is not homogeneous. This is a very old explanation, but like many sober old inductive truths it is not so attractive nowadays as an appeal to the imagination, combined with a good, sturdy, consistent loyalty to some *à priori* postulate, which would have won the hearts of the old schoolmen.

HENRY H. HOWORTH.

30 Collingham Place, Cromwell Road, November 16.

Rock Basins in the Himalayas.

THERE is one statement in the interesting communication of my colleague, Mr. T. D. LaTouche, which seems to require qualification. After a tolerably extensive experience of the Himalayas, I should be inclined to say that rock basins are of fairly frequent occurrence, of all sizes from the largest to the smallest, but they are almost without exception filled with stream deposits, and only occasionally can their formation have been due to glaciers; for they are usually found where there are no traces of glacial action to be seen, and at levels to which we have no reason to suppose that glaciers ever reached. In the hills of eastern Baluchistan, where the rainfall is much less than in the Himalayas, rock basins more or less filled by recent surface deposits are even more common, and here their origin by deformation of the surface can generally be established. The same cause probably accounts for the Himalayan rock basins, as there are abundant proofs that the elevatory movement has been far from uniform, and that the variations in its intensity have been both extensive and often extremely local. There are frequent occurrences of surface deposits which appear to have originally been formed in rock basins, but have since been cut into by the streams, owing to the corrasion of the barrier, and we may attribute the absence of lakes in the Himalayas to the rapid current and large burden carried by the streams, in consequence of which they have been able to fill up the basin, and often to corrade the barrier, as fast as it was formed.

R. D. OLDHAM.

"Composite" Dykes.

PROF. JUDD'S excellent paper in the current issue of the *Quarterly Journal of the Geological Society* (p. 536) calls to my mind some common and similar examples among the "elvans" of Cornwall (which are dykes in the ordinary acceptation of the term), and but little has been published offering some explanation of their bearing on surrounding rocks. I have observed, notably in the district of Cligga Head (nine miles N.W. of Truro), the marked difference between the structures exhibited by dykes in the parts in contact with the rock through which they intrude (in the Cligga instance Devonian slate), and their centre, amounting almost to a rock distinction.

In the appended sketches I have endeavoured to illustrate my meaning from actual instances.

Fig. 1 represents a section of an elvan or dyke outcropping slightly to the north of Cligga promontory, and from its position apparently connected with the main mass of Cligga Head granite. It bursts through the slate. The centre (*b*) of the dyke consists of a rock of homogeneous texture, quartzo-felspathic base, and some scattered porphyritic felspar crystals. The sides (*a a*) in contact with the slate (*s s*) show a rock of apparently similar base, but shot with long acicular crystals of schorl, the whole rock being of a very dark colour, due probably to the presence of wolfram.

Fig. 2 is a section of a very common form of Cornish elvan, consisting of alternate laminæ of granite (*d d*) and "schorl rock," that is, rock consisting of schorl and quartz, generally in about equal proportions (*c c*).

These bands are very common in the slates and in the granitic bosses. Further, an analysis of a typical "schorl rock" of this class showed a silica percentage of 67·6 (*vide* Judd's paper, p. 545), and of a typical granitic band of 74·8 (De La Beche, "Report on Geology of Cornwall, Devon, and West Somerset," p. 189). It is very doubtful, however, if either of the above instances is a case of a dyke putting on such differences in mineralogical and chemical character in its several parts as to amount to a difference of rock species.

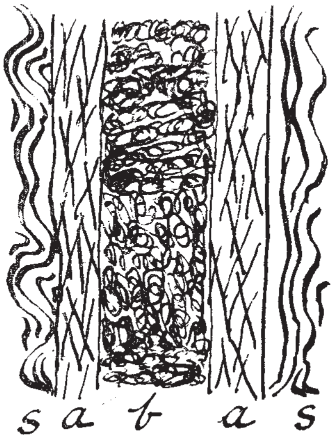

FIG. 1.

As De La Beche points out, the schorl rock may be simply a granite in which the felspar and mica are replaced by schorl. An instance, however, of a rock one may call "a dyke within a dyke" is the Cligga mass itself, which is nothing but a gigantic dyke. De La Beche, in his work above cited (p. 164), has figured it. The dyke is so strikingly split into layers as to appear stratified, the hard comparatively small-grained layers standing out in bold relief from the contiguous layers of more easily decomposed rock with their large porphyritic felspar crystals.

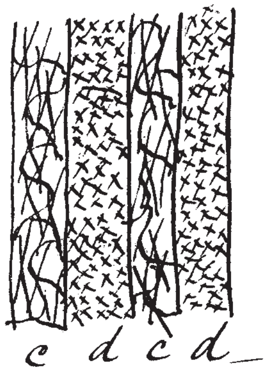

FIG. 2.

Besides the difference in size of the felspar crystals, the harder rock is much darker in colour (being of a red hue) than the softer, which is pale pink and in places whitish. These physical differences, however, count for little in drawing a distinction of rock species between the layers, and I was unfortunately unable to avail myself of any published analyses of the different parts, but their superficial characters are so distinct as

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to render the stratified appearance of the rock very marked at comparatively great distances from them.

There is in many cases a crack marking the junction of contiguous layers.

As an illustration of these "composite" dykes, I append a diagrammatic sketch representing a section of the coast about 200 or 300 yards south of the Cligga promontory, which is very difficult of approach.

A has all the appearance of a bed of sandstone, the strata curved, owing to the intrusion of the dyke *B* (granitic) ; *C* is an old tin burrow. As a matter of fact, each is a granitic dyke, *A* finer grained than *B*, and very like sandstone in all petrological features.

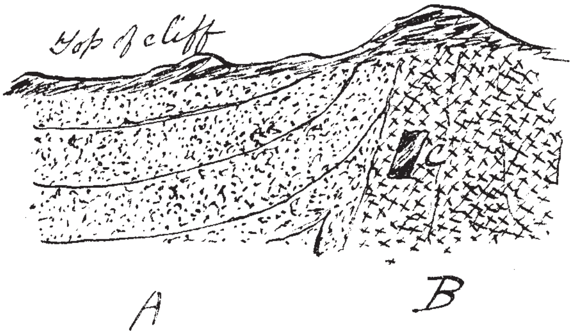

FIG. 3.

The remarkable fact is the apparent stratification of the beds *A*, which are really bands of several dykes—a continuation of those figured at p. 164 in De La Beche's book. He does not seem to have observed this instance, or at any rate does not mention it ; his figure is from the cliff immediately in contact with the Cligga promontory, and north of that I have figured.

Further instances of this very interesting kind of composite dyke would help in many cases to unravel the seeming complexity of such geological features as those I have touched upon in Cornwall. HENRY E. EDE.

45 Walker Terrace, Gateshead-on-Tyne, October 4.

Weismannism.

I NEVER answer reviews, save in so far as they may be misleading on matters of fact. As this is the case with "P. C. M.'s" notice of my "Examination of Weismannism" (NATURE, November 16), I should like to say a few words touching the more important of such matters.

It seems that in seeking to do justice to all sides in the heredity question, I have been too careless in expressing my own view. At all events, any one reading the review must gather from it that I am a Lamarckian engaged in fighting the theories of Prof. Weismann. In the book, however, it is stated that I have been an adherent of the theory of Stirp ever since it was published by Mr. Galton in 1875. It is also stated that this theory is, in my opinion, identical, as regards all main principles, with that of Germ-plasm in the present phase of its numerous metamorphoses. Therefore, far from fighting the Weismannian theory of heredity, I see in all its main features, as it now stands, a "re-publication" of the one which I have held for close upon twenty years.

It is further stated that the only points of much secondary importance wherein I can perceive the two theories to differ are, (*a*), that while Galton confined himself to publishing a theory of Heredity, Weismann proceeded to rear upon this basis (*i.e.*, the hypothesis of "continuity") a further and elaborate theory of organic evolution ; and, (*b*), that Weismann has not gone so far as Galton did in expressly recognising the possibility of an occasional transmission of acquired characters, in faint though presumably accumulative degrees. As regards these two points of difference, I have endeavoured to show, (*a*), that Weismann has now himself withdrawn nearly all his previous generalisations with regard to organic evolution, while largely modifying his theory of heredity ; and, (*b*), that he has only to expand certain hints which he has already given—and which, if expanded, would entail much less modification of his original system than those which he has now made in other parts thereof—in order as fully to recognise as Galton did the possibly occasional transmission of acquired characters.

Hence, such opposition as I have found any reason to express with regard to Weismann's system in the latest phase of its development arises, almost exclusively, against the inordinately speculative character of his method. The history of science furnishes no approach to such a disproportion between deduction and induction.

Thus it seems to me that any writer on Weismannism who aims at impartiality must fail in his aim, if he does not give due prominence to this the most distinctive feature of Weismann's method. And, unless the reviewer is prepared to defend such a method as scientific, he has no reason to quarrel with what he calls my "hard words," since they all have reference to it, and are statements, not of opinions, but of facts.

On the other hand, I have endeavoured by "soft words" to fully recognise the great merit of Weismann's work in constituting the heredity question one of world-wide interest. And any bias that I may have with regard to this question is assuredly on the side of "continuity," although I cannot hold that the subordinate question is closed—*i.e.*, as to whether such continuity can never, under any circumstances or in any degrees, be interrupted. GEORGE J. ROMANES.

Hyères, November 20.

Correlation of Solar and Magnetic Phenomena.

MR. ELLIS, in his letter (NATURE, November 9), has discussed the coincidence between Carrington's observation of a solar outburst in 1859 and the magnetic movements observed at Kew and Greenwich. He comes to the conclusion that the disturbance of the magnets corresponding to this outburst was small, and that, although many greater magnetic movements have occurred since, no corresponding manifestation has been seen, although the sun has been so closely watched.

He appears to have overlooked an observation made at Sherman, by Prof. Young, which shows a very striking *series* of coincidences, and which is described in his work, "The Sun" (p. 156), in the following words:—"On August 3, 1872, the chromosphere in the neighbourhood of a sun-spot, which was just coming into view around the edge of the sun, was greatly disturbed on several occasions during the forenoon. Jets of luminous matter of intense brilliance were projected, and the dark lines of the spectrum were reversed by hundreds for a few minutes at a time. There were three especially notable paroxysms at 8.45, 10.30, and 11.50 a.m., local time. At dinner the photographer of the party, who was making our magnetic observations, told me, before knowing anything about what I had been observing, that he had been obliged to give up work, his magnet having swung clear off the scale. Two days later the spot had come round the edge of the limb. On the morning of August 5, I began observations at 6.40, and for about an hour witnessed some of the most remarkable phenomena I have ever seen. The hydrogen lines, with many others, were brilliantly reversed in the spectrum of the nucleus, and at one point in the penumbra the C line sent out what looked like a blowpipe jet, projecting toward the upper end of the spectrum, and indicating a motion along the line of sight of about 120 miles per second. The motion would die out and be renewed again at intervals of a minute or two. . . . . The disturbance ceased before eight o'clock, and was not renewed that forenoon. On writing to England, I received from Greenwich and Stonyhurst, through the kindness of Sir G. B. Airy and Rev. S. J. Perry, copies of the photographic magnetic records for those two days. . . . . On August 3, which was a day of general magnetic disturbance, the paroxysms I noticed at Sherman were accompanied by peculiar twitches of the magnet in England. Again, August 5 was a quiet day, magnetically speaking, but just during that hour, when the sun-spot was active, the magnet shivered and trembled. So far as appears, too, the magnetic action of the sun was instantaneous. After making allowance for longitude, the magnetic disturbance in England was strictly simultaneous, so far as can be judged, with the spectroscopic disturbance seen on the Rocky Mountains."

These observations of Prof. Young's seem to invalidate Mr. Ellis's statement that "no second occurrence similar to that of 1859 has come to light," and that although there undoubtedly exists a relation between sun-spots and magnetism, "it has not yet been found possible to trace direct correspondence in details."

Cambridge, November 12. A. R. HINKS.

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